that the new star had put on the appearance presented ordinarily by the so-called planetary nebulæ.

Of all the lines chronicled by Cornu and Vogel during its stellar stage, only one remained, that, namely, which the latter observer showed to be constantly increasing in brightness while all the rest were waning, and which, moreover, was coincident in position in the spectrum with that observed in the majority of the nebulæ.

The observations of such rare phenomena as the so-called new stars are of such vast importance, and will no doubt ultimately provide us with a clue to so many others of a different order, that we may well congratulate ourselves that this Nova was so well watched, and that there is such perfect completeness and unity in the chain of recorded facts.

It should have been perfectly clear to those who thought about such matters that the word star in such a case is a misnomer

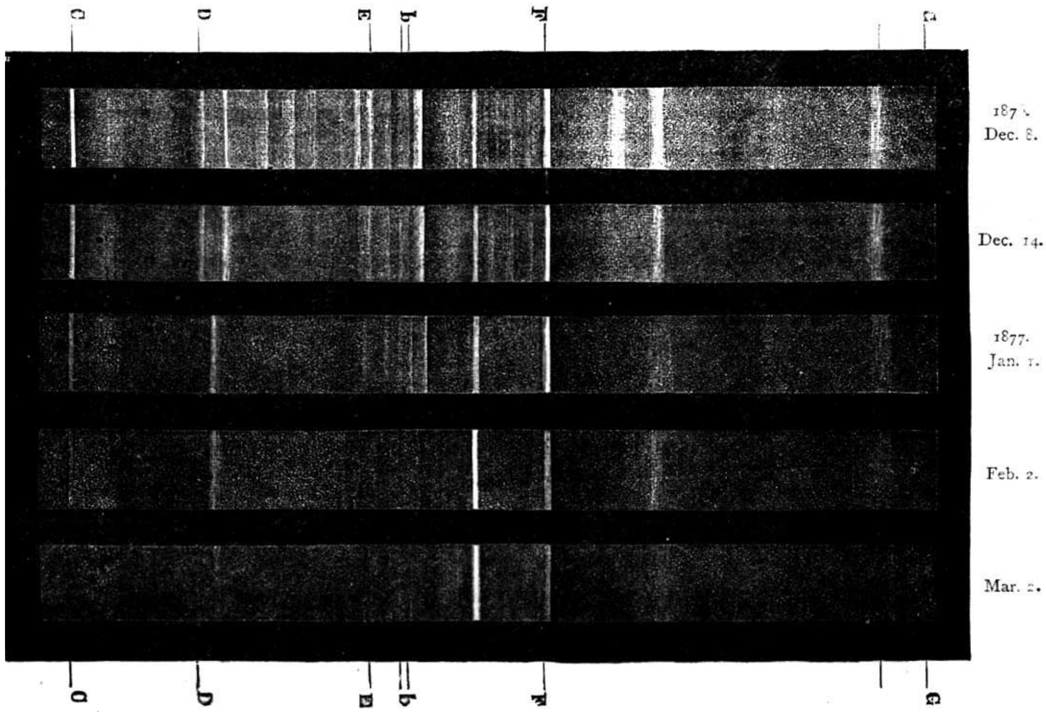

FIG. 26.—Vogel's spectrum of Nova Cygni.

from a scientific point of view, although no word would be better to describe it in its popular aspect. The word is a misnomer for this reason. If any star, properly so called, were to become "a world on fire," were to "burst into flames," or in less poetical language, were to be driven either into a condition of incandescence absolutely, or to have its incandescence increased, there can be little doubt that thousands or millions of years would be necessary for the reduction of its light to the original intensity.

Mr. Croll has shown that if the incandescence observed came for instance from the collision of two stars, each of them half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, light and heat would be produced which would cover the present rate of the sun's radiation for a period of 50,000,000 years.

A very different state of affairs this from that which must have taken place in any of the Novas from the time of Tycho to our own, and the more extreme the difference the less can we be having to deal with anything like a star properly so called.

The very rapid reduction of light in the case of the new star in Cygnus was so striking that I at once wrote to Mr. Hind to ask if any change of place was observable, because it seemed obvious that if the body which thus put on so suddenly the chromospheric spectrum were single, *it might only weigh a few tons or even hundredweights*, and being so small might be very near us. No motion, however, was perceptible, and Dr. Ball has since stated that he could detect no parallax.

We seem driven, then, from the idea that these phenomena are produced by the incandescence of large masses of matter, because if they were so produced, the running down of brilliancy would be exceedingly slow. J. NORMAN LOCKYER

(*To be continued.*)

FLAME CONTACT, A NEW DEPARTURE IN WATER HEATING[1]

It is my intention to prove to you on theoretical grounds, and also by experimental demonstration, in such a manner as will admit of no possible doubt, that the present accepted system of water heating, by gaseous or other fuel, is a very imperfect means for an end, and is, both in theory and practice, essentially faulty. My statements may appear bold, but I come prepared to prove them in a manner which I think none of you will question, as the matter admits of the simplest demonstration. I will, in the first place, boil a specified quantity of water in a flat-bottomed vessel of copper; the time required to boil this you will be able to take for yourselves, as the result will be visible by the discharge of a strong jet of steam from the boiler.

[1] A Paper read by Thomas Fletcher, F.C.S., at the Gas Institute Meeting, London, June 9.

I will then take another copper boiler of the same form, but with only one-half the surface to give up its heat to the water, and will in this vessel boil the same quantity of water with the same burner in a little over one-half the time, thus about doubling the efficiency of the burner, and increasing the effective duty of the heating surface fourfold, by getting almost double the work from one-half the surface.

The subject is a comparatively new one, and my experiments are far from complete on all points, but they are sufficiently so to prove my case fully. As no doubt you are all aware, it is not possible to obtain flame contact with any cold, or comparatively cold, surface. This is readily proved by placing a vessel of water with a perfectly flat bottom over an atmospheric gas-burner: if the eye is placed on a level with the bottom of the vessel a clear space will be seen between it and the flame. I cannot show this space on a lecture-table to an audience, but I can prove its existence by pasting a paper label on the bottom

of one of the boilers, and exposing this to the direct impact of a powerful burner during the time the water is being boiled, and you will see that it comes out perfectly clean and uncoloured. Now it is well known that paper becomes charred at a temperature of about 400° F., and the fact that my test-paper is not charred proves that it has not been exposed to this temperature, the flame being, in fact, extinguished by the cooling power of the water in the vessel. I need hardly remind you that the speed with which convected or conducted heat is absorbed by any body is in direct ratio to the difference between its own temperature and that of the source of heat in absolute contact with it; and therefore, as the source of the heat taken up by the vessel is nothing but unburnt gases, at a temperature below 400° F., the rate of absorption cannot, under any circumstances, be great, and the usual practice is to compensate for this inefficiency by an enormous extension of surface in contact with the water, which extension I will prove to you is quite unnecessary. You will see I have here a copper vessel with a number of solid copper rods depending from the lower surface; each rod passes through into the water space and is flattened into a broad head, which gives up its heat rapidly to the water. My theory can be stated in a few words: The lower ends of the rods, not being in close communication with the water, can, and do attain, a temperature sufficiently high to admit of direct flame contact, and as their efficiency, like that of the water surface, depends on the difference between their own temperature and that of the source of heat in absolute contact with them, we must, if my theory is correct, obtain a far greater duty from them. I do not wish you to take anything for granted, and although the surface of the rods, being vertical, can only be calculated for evaporating power at one-half that of a horizontal surface, as is usual in boiler practice, my margin of increased duty is so great that I can afford to ignore this, and to take the whole at what its value would be as horizontal surface, and still obtain a duty 50 per cent. greater from a surface which is the same in area as the flat-bottomed vessel on the fire side, but having only one-third the surface area in contact with the water. I do not, of course, profess to obtain more heat from the fuel than it contains, but simply to utilise that heat to the fullest possible extent by the use of heating surfaces, beyond comparison smaller than what have been considered necessary, and to prove not only that the heating surfaces can be concentrated in a very small area, but also that its efficiency can be greatly increased by preventing close water contact, and so permitting combustion in complete contact with a part of the heating surface. I will now boil 40 ounces of water in this flat-bottomed copper vessel, and, as you will see, sharp boiling begins in 3 minutes 15 seconds from the time the gas is lighted. The small quantity of steam evolved before this time is of no importance, being caused partly by the air driven off from the water and partly from local boiling at the edges of the vessel owing to imperfect circulation. On the bottom of this vessel is pasted a paper label which you will see is untouched by the flame owing to the fact that no flame can exist in contact with a cold surface.

It may be thought that, owing to the rapid conducting power of copper, the paper cannot get hot enough to char. This is quite a mistake, as I will show you by a very curious experiment. I will hold a small plate of copper in the flame for a few seconds, and will then hold it against the paper. You will see that, although the copper must of necessity be at a temperature not exceeding that of the flame, it readily chars the paper. We can, by a modification of this experiment, measure the depth of the flameless space, as the copper, if placed against the paper before it has time to be previously heated, will, if not thicker than 1/40 inch, never become hot enough to discolour the paper, showing that the flame and source of heat must be below the level of a plate of metal this thickness.

In repeating this experiment I must caution you to use flour paste, not gum, which is liable to swell and force the paper past the limit of the flameless space, and also to allow the paste to dry before applying the flame, as the steam formed by the wet paste is liable also to lift the paper away and force it into the flame. I will now take this vessel, which has only one-half the surface in contact with the water, the lower half being covered with copper rods, 3/16 inch diameter, ½-inch centres apart, and 1½ inch long, and you will see that with the same burner as before, under precisely the same conditions, sharp boiling takes place in 1 minute 50 seconds, being only 13 seconds more than half the time required to produce the same result with the same quantity of water as in the previous experiment. Although the water surface in contact with the source of heat is only one-half that of the first vessel, and the burner is the same, we can see the difference not only in the time required to boil the 40 ounces of water, but also in the much greater force and volume of steam evolved when boiling does occur. With reference to the form and proportions of the conducting rods, these can only be obtained by direct experiment in each case for each distinct purpose. The conducting power of a metallic rod is limited, and the higher the temperature of the source of heat, the shorter will the rods need to be, so as to insure the free ends being below a red heat, and so prevent oxidation and wasting. There are also other reasons which limit the proportions of the rods, such as liability to choke with dirt and difficulty of cleaning, and also risk of mechanical injury in such cases as ordinary kettles or pans; all these requirements need to be met by different forms and strengths of rods to insure permanent service, and, as you will see further on, by substituting in some cases a different form and type of heat conductor. To prove my theory as to the greater efficiency of the surface of the rods in contact with the flame as against that in direct contact with the water, I have another smaller vessel which, including the rods, has the same total surface in contact with the flame, but only one-third the water surface as compared with the first experiment. Using again the same quantity of water and the same burner we get sharp boiling in 2 minutes 10 seconds, being an increase of duty of 50 per cent., with the same surface exposed to the flame. The rods in the last experiment form two-thirds of the total heating surface, and if we take, as I think for some careful experiments we may safely do, one-half the length of the rods to be at a temperature which will admit of direct flame contact, we have here the extraordinary result that flame contact with one-third of the heating surface increases the total fuel duty on a limited area 50 per cent. This really means that the area in contact with flame is something like six times as efficient as the other. In laboratory experiments it is necessary not only to get your result, but to prove your result is correct, and the proof of the theory admits of ready demonstration in your own laboratories, although it is unfit for a lecture experiment, at all events in the only form I have tested it. If you will take two ordinary metal ladles for melting lead, cover the lower part of one of these with the projecting rods or studs and leave the other plain, you will find on melting a specified quantity of metal in each that the difference in duty between the two is very small. The slight increase may be fully accounted for by the difference in the available heating surface reducing the amount of waste heat passing away, and this proves that flame-contact, and therefore quick absorption of heat, takes place on plain surfaces as soon as these are above a certain temperature, which, in a metal ladle, very soon occurs. What the temperature is which admits of flame-contact I have, as yet, not been able to test thoroughly, and it will need some consideration how the determination of this is to be correctly made; at the same time it is a question in physics which should be capable of being answered.

Let us now take the other side of the question. If the efficiency of a surface depends on flame contact, there must of course be flame, or at least gases of an extremely high temperature, and we therefore cannot expect this extraordinary increase of efficiency in any part of our boiler except where flame exists, and if these projectors are placed in a boiler, anywhere except in contact with flame, their efficiency must be reduced to that of ordinary heating surface. They are, of course, useful, but only in the same way as ordinary flue surface. When we come to boilers for raising steam, which have to stand high pressures, we come to other difficulties of a very serious nature, which require special provision to overcome them. To put such rods as I have referred to in a boiler-plate necessitates the plate being drilled all over with holes, causing a dangerous source of weakness, as the rods cannot be used as stays; further than this, they would render really efficient examination a matter of extreme difficulty, and would be liable to give rise to frequent and almost incurable leakages; but there is, fortunately, a very simple way to overcome this difficulty. I have found that rods or points, such as I have described, are not necessary, and that the same results can be obtained by webs or angle-ribs rolled in the plates. My experiments in this direction are not complete, and at present they tend to the conclusion that circular webs, which would be of the greatest efficiency in strengthening the flues, are not so efficient for heating as webs running lengthwise with the flue, and in a line with the direction of the flame. This point is one which I am at present engaged in testing with experimental

boilers of the Cornish and Lancashire types, and as, with gas, we have a fuel which renders every assistance to the experimenter, it will not take long to prove the comparative results obtained by the two different forms of web. Those of you who have steam-boilers will, no doubt, know the great liability to cracking at the rivet-holes in those parts where the plates are double. This cracking, so far as my own limited experience goes, being usually, if not always, on the fire side, where the end of the plate is not in direct contact with the water—where it is, in fact, under the conditions of one of the proposed webs—I think we may safely come to the conclusion that this cracking is caused by the great comparative expansion and contraction of the edge of the plate in contact with the fire; and it will probably be found that if the plates are covered with webs the whole of the surface of the plates will be kept at a higher and more uniform temperature, and the tendency to cracks at the rivet-holes will be reduced. This is a question not entirely of theory, but needs to be tested in actual practice.

There is another point of importance in boilers of the locomotive class, and those in which a very high temperature is kept in the fire-box, and this is the necessity of determining by direct experiment the speed with which heat can safely be conducted to the water without causing the evolution of steam to be so rapid as to prevent the water remaining in contact with the plates, and also whether the steam will or will not carry mechanically with it so much water as to make it objectionably wet, and cause priming and loss of work by water being carried into the cylinders. I have observed in the open boilers I use that when sufficient heat is applied to evaporate 1 cubic foot of water per hour from 1 square foot of boiler surface, the bulk of the water in the vessel is about doubled, and that the water holds permanently in suspension a bulk of steam equal to itself. I have, as yet, not had sufficient experience to say anything positively as to the formation or adhesion of scale on such surfaces as I refer to, but the whole of my experimental boilers have up to the present remained bright and clean on the water surface, being distinctly cleaner than the boiler used with ordinary flat surfaces. It is, I believe, generally acknowledged that quick heating and rapid circulation prevents to some extent the formation of hard scale, and this is in perfect accord with the results of my experiments. The experiments which I have shown you I think demonstrate beyond all question that the steaming-power of boilers in limited spaces, such as our sea-going ships, can be greatly increased; and when we consider how valuable space is on board ship, the matter is one worthy of serious study and experiment. It may be well to mention that some applications of this theory are already patented.

I will now show you as a matter of interest in the application of coal gas as a fuel how quickly a small quantity of water can be boiled by a kettle constructed on the principle I have described, and to make the experiment a practical one I will use a heavy and strongly-made copper kettle which weighs $6\frac{1}{2}$ lbs., and will hold when full one gallon. In this kettle I will boil a pint of water, and, as you see, rapid boiling takes place in 50 seconds. The same result could be attained in a light and specially-made kettle in 30 seconds, but the experiment would not be a fair practical one, as the vessel used would not be fit for hard daily service, and I have therefore limited myself to what can be done in actual daily work rather than laboratory results, which, however interesting they may be, would not be a fair example of the apparatus in actual use at present.

THE CRATERS OF MOKUAWEOWEO, ON MAUNA LOA[1]

DURING last year I was engaged for many months in surveying lands on Mauna Hualalai and Mauna Loa, in Hawaii, and in that way had an opportunity of making investigations of craters and lava flows that may be of interest to those studying volcanic phenomena.

It would seem that, as the best histories are those written long after the events which they record, when all the reports of eye-witnesses can be carefully examined, so the best descriptions of volcanic action may be obtained long after eruptions, by carefully investigating the records indeliby inscribed in the rocks.

The ascent of Mauna Loa is so seldom made that a brief account of my excursions may be interesting.

[1] By J. M. Alexander, from the *Hawaiian Commercial Advertiser* of October 1885.

On September 1, 1885, I set out in company with Mr. J. S. Emerson, of the Hawaiian Government Survey, to ascend that mountain from the table-land east of Hualalai, along the south side of the lava-flow of 1859, which, as many will remember, was visited by a party from Oahu College. We were provided with mules for riding and pack-donkeys, and accompanied by several natives, including a so-called guide, who lost himself and delayed us over a day in searching for him.

Our route led first through a narrow belt of forest, consisting of mamane, ohia, and sandalwood trees; then through a scanty vegetation of ohelos and the beautiful *Cyathodes Tameiameiæ*, and at last beyond the limits of vegetation, without a vestige even of moss or lichen, over a wonderful and awful billowy waste of "pahoehoe" lava, traversed by tracts of "aa" and deep chasms.

At about two-thirds of the distance towards the summit we passed the rugged crater hill from which the outbreak of 1859 had issued, and here our path was strewed with pumice and "Pele's hair" from that eruption. There was an enormous quantity of lava poured forth from the small fissure of this crater, forming a stream from half a mile to two miles wide, and reaching nearly thirty miles to the ocean at Kiholo. Lower down I counted eighteen species of ferns and a dozen kinds of phenogamous plants already growing on this flow. In this vicinity the caverns contained many carcasses of wild goats. In one further south I counted eighty of their skeletons and decaying bodies. They had probably leaped in for shelter, and had been unable to leap out.

When near the summit our guide warned us to descend, because of an approaching storm; but Mr. Emerson and I, anxious to accomplish the object of our journey, set out without him through the driving rain that soon turned into hail and then into snow, marking our route with flags so that we might be able to find our way back. In a short time we reached the brink of the vast crater of Mokuaweoweo, filled with fog and surrounded by frightful precipices. Along this brink were numerous deep fissures filled with ice and water, the beginning of cleavage for avalanches into the crater. Here, and for a quarter of a mile below, we observed many rocks of a different kind from the surface lavas, solid, flinty fragments of the foundation walls, weighing from fifty pounds to a ton, which had formerly fallen down upon the crater floor and had afterwards been hurled out during eruptions. I noticed similar rocks around the summit craters of Hualalai. It would be unsafe to approach the crater at this place during eruptions, when such brickbats were flying.

We returned to our camp about noon, and sent the poor animals, which had stood all night in the icy wind tied to jagged rocks, in the care of the guide down the mountain; and with the help of one native, with much difficulty, carried a tent and supplies to the summit.

At evening the fog lifted and gave us a glimpse of the craters. Immediately below us lay the central crater, surrounded by almost perpendicular walls, with a pahoehoe floor streaked with grey sulphur cracks, from hundreds of which there issued columns of steam, and with a still smoking cone in the south end. Beyond this central crater on the south rose a high plateau, and beyond this plateau still further south we saw an opening into another crater small and deep. In the opposite direction, north of the central crater, appeared another higher crater like an upper plateau, from which a torrent of lava had once poured into the central crater, and north of this again another crater, like a still higher plateau, from which also lava had flowed south.

Thus it was evident, as appeared more clearly by subsequent investigation, that Mokuaweoweo is not simply one crater, but a series of four or five craters, the walls of which have broken down, so that they have flowed into each other.

The crater of Haleakala, on Maui, was probably formed in a similar manner out of several ancient craters which have broken into each other. These vast chasms may well be called calderas, as has been recommended by Captain Dutton. On Hualalai there is a series of craters having the same relative position as those of Mokuaweoweo, and crowded so close together as to be almost broken into one. On the older mountains, like that of West Maui, such congeries of craters have evidently formed the starting-points for deep valleys, which the rain torrents, leaping down their lofty walls, have torn out through concentric layers of lava to the sea. Just before sunset we saw the splendid phenomenon of the "Spectre of the Brocken" (Hookuaka),